Review of Robert D. Hinshelwood, *Research on the Couch: Single-Case studies, Subjectivity and Scientific Knowledge.*

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Robert Hinshelwood's new book is a valuable contribution to the debates concerning the scientific status of psychoanalysis, and about how psychoanalytic theories can find a rational justification. Hinshelwood's primary interest is in the validity of psychoanalytic theories of the mind, not in the 'evidence-based' measures of treatment outcome which now receive most attention. He has concerns about the proliferation of psychoanalytic theories, and about the difficulty in selecting between competing theories in the way that he believes natural science does much more successfully. He describes the present situation where there are overlapping and ill-defined psychoanalytic theories, clustered in different schools and followings, with little capacity for resolution of differences, as a Tower of Babel. In this view Hinshelwood is taking up concerns previously set out in studies of psychoanalytic practice for example by Robert Wallerstein (1988) and David Tuckett (2005). His book proposes a critical procedure by which competing theories can be subjected to decisive empirical test.

Hinshelwood believes that the fundamental critique of psychoanalysis first advanced by Karl Popper in 1963 , and developed by Adolf Grünbaum in 1984 and 1993 has never been adequately refuted. He accepts as an essentially valid model of scientific procedure Popper's 'hypothetico-deductive method', originally set out in *The Logic of Scientific Discovery* in 1935. According to this, scientific theories set out generalisations, or universal laws, which are subjected to the test of empirical falsification through controlled experiments or observations. This model contrasts with the 'inductive method' according to which knowledge is advanced through the accumulation of positive evidence for theories, which Popper believed cannot generate scientific knowledge. Popper attacked as 'pseudo-sciences' those theoretical systems,

notably Marxism and psychoanalysis, which pretended to the status of a science but failed to meet the requirement of testing by empirical falsification.[1]

Hinshelwood holds that it is because psychoanalysis has not been able to respond adequately to this 'modern' view of scientific method that it has found it difficult to justify itself. He implies that previously psychoanalysis had found itself on stronger ground, but says little about this earlier phase.[2] The most significant critiques of psychoanalysis in a book edited by Sidney Hook (1959) and in Popper's writing, date from 1959 and 1963, were advanced in the context of a predominantly 'positivist' climate in Anglo-American philosophy. To call these positions 'modern' glosses over much of the debate on the nature of science that has since taken place. Nevertheless, this critique provides Hinshelwood's main point of orientation, and Popper and Grünbaum have more index citations than any author other than Freud. What he wishes to show is that psychoanalysis can, if its procedures are appropriately reformulated, meet the central Popperian criterion of scientific truth, which is that theories must be validated by the criterion of empirical falsifiability.

It is important in setting out an argument like this that an adequate account is given of the psychoanalytic theories and methods which are to be tested. There are two main respects in which the Hinshelwood sets out to do this. The first is in his emphasis on the clinical case study, and often the 'single case study', as the primary source of psychoanalytic knowledge. Against the common misrepresentation of the natural sciences as essentially quantitative in their approach, Hinshelwood argues that many fundamental scientific discoveries have been made from the investigation, often in laboratory conditions, of single (or very few) instances. An example he gives of falsification by a single instance is Columbus's voyage of 1492, which allegedly showed through a single natural experiment that the earth was round, and not flat. In fact it appears that neither Columbus nor those of learning in his time any longer

[1] Popper's own remarks on this topic were extremely brief, in *Conjectures and Refutations (1963)* but his critique was carried forward, in various forms by other writers, such as Cioffi 1998, Gellner 1985, and Grünbaum, op cit.

[2] John Forrester's work on the influence of psychoanalysis in Cambridge in the 1930s does discuss this receptive climate, but in historical rather than philosophical terms. See his various papers accessible at http://www.hps.cam.ac.uk/people/forrester/at

believed in 'flat earth' theory. But although this example does not seem a good one, the main point about intensive forms of investigation can be well documented from the history of the natural sciences. Hinshelwood's inference is that since single case studies generate valid knowledge in the natural sciences, they should equally be expected to do so in psychoanalysis. The question then is how to design such a model of validation.

The second way in which Hinshelwood aims to describe psychoanalysis as it is (rather than abandon its principles to make its research more respectable) is his insistence that the dimensions of both cause and meaning are necessary to psychoanalytic explanation. He rightly argues against an exclusive interest in relations of cause and effect, and against giving attention only to 'subjective meanings' – the 'hermeneutic' perspective. The hermeneutic perspective disavows the significance of 'objective' knowledge in psychoanalysis – subjective coherence rather than truth becomes its goal. Hinshelwood associates this with a 'postmodern' orientation, in which all representations of reality are understood to be interpretations, whether by individuals or cultures. In that perspective, psychoanalysis is seen not as a search for psychic truth by analyst and patient, but rather as the co-construction between them of descriptions and narratives which are found to be satisfying. This is a recognisable position in modern psychoanalysis (for example within the 'relational school' in the USA), but Hinshelwood rejects it. Like other Kleinians, (e.g. O'Shaughnessy 1994) he holds that psychoanalytic propositions about psychic reality <u>can</u> be ascertained to be true or false.

In defence of the view that the psychoanalytic view of the mind must take account of both reasons or subjective meanings, and causes, Hinshelwood draws on the philosopher Donald Davidson's view that 'reasons' can themselves be causes of human actions. One therefore need not choose between 'reasons' and causes' as if these were incompatible modes of explanation.[3]

What is necessarily distinctive about psychoanalysis (compared with the natural sciences) is that it depends on ways of thinking which are shared by its investigators

[3] Incidentally Hinshelwood mistakenly attributes to Paul Ricoeur the view that psychoanalysis is concerned with meanings only. Ricoeur (1977) argues that both dimensions must be part of psychoanalytic explanation.

(analysts) and its objects of investigation (analysands). He argues that in the natural sciences the instruments used for observation and measurement are distinct from the 'objects' which are being studied. The validity of the means of investigation thus depends on theories distinct from those used to explain the phenomena being studied. In psychoanalysis, however, the same theories about unconscious processes are used both to understand mental states, and the interactions in the transference by which these are observed. What valid test of the existence of an unconscious process can there be, Hinshelwood asks, if its existence is presupposed in its method of discovery?

Hinshelwood tries to avoid this risk of circularity by differentiating between two elements of psychoanalytic investigation – one concerned with content and the other with process. He proposes, drawing on Wallerstein, that a 'hermeneutic' method be used to formulate hypotheses concerning the 'content' or metapsychological structure of a patient's mind, and that a 'causal' method be used to test such hypotheses in the psychoanalytical process which consists of transference-countertransference interactions, analysts' interpretations, and patients' responses to them. The idea that the existence of, and causal relations between the entities postulated by a scientific hypothesis needs to be tested by independent methods of observing them is surely correct. What is less clear is that there is anything distinctive in this regard about psychoanalysis, compared with other kinds of scientific inquiry.[4]

One of Hinshelwood's purposes here is to respond to Grünbaum 's principal criticism of Freud's scientific method. He argued against Freud's 'tally principle' that the truth of interpretations could not be ascertained by the analysing's agreement with them, because an analysing's response is subject to the influence 'by suggestion' of the analyst. This is a kind of 'circularity' related to that which Hinshelwood has identified. According to Grünbaum an analysand is not free to make use of criteria of evaluation distinct from those implied in interpretations (For example, an analysand's disagreement is liable to be interpreted as 'resistance'.) [5]

[4] The instruments of measurement used in physics or chemistry do not disavow the theories of physics or chemistry in their construction. Care is merely taken to make sure that the outcome of an observation is not presupposed in its design. Similarly in psychoanalysis, while one cannot use transference to establish the existence of transference, one can make use of the transference to establish the existence in patients of various unconscious states of mind, such as Oedipal jealousy.

[5] Grünbaum 's description is recognisable, but I doubt that his account corresponds to good analytic practice (Wollheim 1993). Discriminations are surely made between responses to interpretations which

Having set out these issues, Hinshelwood sets out a model through which clinical hypotheses can be formulated and tested in a way which will satisfy the criteria of falsifiability. He gives examples both from published clinical literature, and from his own clinical work. He proposes that one can test 'theoretical conjectures' (to use Popper's term) from the written, preferably verbatim, clinical record of a session, by reference to a patient's response to an interpretation. He gives as an example dispute between Kleinians, who hold that 'it is necessary to interpret envy in severe disorders', and those who hold that 'what Kleinians interpret as envy can be understood as simply deriving from other negative emotions, notably frustration and jealousy.' He suggests a procedure to resolve whether envy is or is not evident, within a segment of clinical material. If it is, he infers, then the existence of envy is supported by this test of falsification. Hinshelwood's contention is that a hypothesis can be justifiably rejected if a single countervailing instance to it can be found.

The limitation of this argument for psychoanalysis is not that such tests of validity are not possible, but rather that the major theoretical differences are not about the existence or otherwise of a state of mind such as envy but rather about its pervasiveness, its origins and its consequences. The scope of application of a concept, and its explanatory value, are established not by a single instance, but in research programmes over periods of time. It is true that new ideas in psychoanalysis (like those of destructive narcissism, or the heuristic value of the countertransference) often enter the field through the presentation of an exemplary case example, but their value becomes established only through their being found to have useful clinical application.

The broad principle that hypotheses need to be tested by empirical falsification should not be contentious for psychoanalysts. Indeed such procedures correspond to what is found in good practice. In their clinical work, analysts surely evaluate the validity of their understandings and interpretations, by continuing observation of their patients'

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are merely compliant, and those which signify real understanding. Analysts decide whether an analysis is going well by reference to whether a patient's own capacities for feeling and thought has been enhanced, not by how far their previous theories are being confirmed. New analytic ideas have most often emerged in response to discordance between prior theoretical beliefs, and clinical evidence. (Rustin 2001, Hughes 2004.)

states of mind and their responses to what is said to them. How could progress be made in understanding patients without continuous monitoring of the validity of analysts' conjectures? Such procedures of evaluation are equally central to clinical research, when the understanding of a particular patient gives rise to the recognition of a phenomenon of a new *kind,* perhaps a hitherto unrecognised kind of psychopathology.

In a significant sense, we all are, or need to be, Popperians now, in the sense that it is essential to any scientific process that discriminations be made, on the basis of factual evidence of a relevant kind, between true and false hypotheses. Without such procedures, there can be nothing but mere opinion, with all the departures from rational practice that Hinshelwood and others have noted can and do occur in the psychoanalytic field.

Some disagreements

Nevertheless, for all the intended rigour of this book's approach, there are some respects in which I think it is mistaken. The main problem lies in the limitations of the Popperian approach to scientific method which Hinshelwood has adopted as his standard. He takes too little account of what has happened in the understanding of the sciences since Popper's contribution was made cades ago.

He does provide some discussion of one essential point of reference for these debates, Thomas Kuhn's *The Structure of Scientific Revolutions* (1962), but he takes insufficient account of this. More important than the philosophical arguments about validity and proof in which Popper and Kuhn and their respective allies have engaged is the change of perspective which Kuhn's work initiated. Kuhn was primarily a historian not a philosopher of science. His approach was descriptive rather than legislative and normative. He was not so much interested in the criteria which might differentiate good science from bad science, as in describing and explaining the actual practices of scientists.

Popper believed that the accumulation of scientific knowledge took place through the competition of rival theories. Because theories necessarily make universal claims,

from which predictions can be inferred, empirical falsification could decisively resolve differences between them. The intellectual context of this view was the success of physics in transforming the understanding of nature, both in the seventeenth century (e.g. Newton and Galileo) and in the early twentieth centuries (Einstein). Hinshelwood holds that arguments between differing psychoanalytic theories should be similarly resolved by tests of the causal relations which they entail.

But in Kuhn's account the crucial unit of analysis for understanding the sciences was not the truth or falsity of a specific hypothesis, but rather the explanatory power of an 'paradigm'. A paradigm is more than a specific theory. It defines a field of study, its 'key 'objects' and entities, its appropriate methods'. Kuhn wrote of 'gestalt switches' taking place between paradigms, [6] making a crucial distinction between 'revolutionary' and 'normal' science. A 'scientific revolution' takes place when the linked concepts and theories which comprise a paradigm are abandoned, and are replaced by another set of ideas. Kuhn suggests that this may redefine a field of study, in a way that may bring entirely new 'objects' into view. (Freud's unconscious was just such a new 'object'.) 'Normal science' consists of the solving of the routine 'puzzles' which are generated by the framing propositions of a new paradigm. An example is the evolutionary science' which has been conducted for 150 years on the foundations of Darwin's theory. Within an established science, few practising scientists concern themselves debating the validity of its core propositions, which they take as already established. Kuhn (2000) described what took place in normal science as a process of 'speciation' - the differentiation of theories and classifications to take account of the variety of phenomena which a paradigm encompassed.

Imre Lakatos, an associate of Popper cited by Hinshelwood, accepted Kuhn's holistic and developmental perspective, but sought to retain the criterion of falsifiability. For him the crucial unit of analysis was not the paradigm, but the research programme. He acknowledged the complex and holistic nature of such programmes, and the substantial evidence which they accumulated, but saw that they were unlikely to be abandoned in response to a particular empirical anomaly, as Popper's falsification criterion suggested they should be. The crucial issue for Lakatos was whether a

[6] (Stephen Toulmin, from a broadly sympathetic position, argued that the process of change is usually a more long-drawn out one than this.)

research programme took a 'progressive' or a 'degenerative' form. 'Progressive problem shifts' were those which extended the scope of a programme in empirically testable ways. The 'degenerative' kinds were those which a programme defended itself against negative evidence through conceptual redefinition. (Popper asserted that this practice was endemic to psychoanalysis). The problem is that whether a programme is 'progressive' or otherwise can only be decided in the long run, since it becomes a matter of judgement how much significance to accord to particular anomalies. Within a well-established research programme, it is often more rational to take note of an empirical problem as one to be attended to once improved research methods become available, than to discard an entire theoretical framework on its account.

In this light, the crucial question to ask is whether psychoanalysis constitutes a 'research programme' of an essentially progressive or degenerative kind. Here one is struck by the dysjunction within two different areas of Hinshelwood's own work. His new book sets out a philosophical critique of the methodological deficiences of psychoanalysis, implying that it lies closer to the 'degenerative' end of Lakatos's spectrum of research programmes. But on the other hand, his magisterial *Dictionary of Kleinian Thought*, is a fully-referenced account of the Kleinian 'branch' of psychoanalytic thought, encompassing both its theory and clinical technique. This book and its successors (Hinshelwood 1994, , Spillius et al 2012) are surely exemplary descriptions of the record of a 'progressive' research programme[7] . In each stage of its development of this programme, clinical researchers have surely tested their conjectures against clinical evidence, in an implicitly 'Popperian' spirit [8]

A similarly 'progressive' account can be given of the development of child psychotherapy, as a sub-field within psychoanalysis. Much 'speciation' has occurred within it, for example the investigations of psychotherapy with severely deprived children, through clinical research. There has been a significant accumulation of knowledge, by a generation of child psychotherapists, whose discoveries have in their turn enriched clinical practice. (Rustin 2009).

[7] Or more precisely sub-programme within the larger field of knowledge of psychoanalysis.
[8] I have sought to demonstrate how this happens, in various papers (Rustin 2001, 2007, 2009).

Historical and sociological studies of science have brought recognition of the diversity of scientific practices, whereas by contrast philosophical approaches concerned primarily with issues of validity usually represent 'science' as a unified field. In Popper's work, physics provided the ideal-typical model by which criteria of validity are set. Hinshelwood often refers to 'science' and 'natural science' as if these are unproblematic categories, and displays little interest in the specificities of any science other than psychoanalysis. But if one holds that the sciences are diverse in their objects and methods of study (Galison and Stump 1996), although with some principles common to all of them, it is important to clarify with which sciences psychoanalysis is most relevantly compared. For example, where physics is based on extraordinarily powerful laws of universal, indeed cosmic application, Darwinian biology and ecology investigates vast fields of particulars, concerning species and their interrelations.

Psychoanalysis is unlike other sciences in being concerned not only with differences between kinds within its field of study, but also with the unique qualities of individuals as such. Its concern with the <u>particulars</u> as well as with the <u>kinds</u> of human experience is what gives psychoanalysis the qualities of an art as well as a science. Hinshelwood implicitly supports this view when he insists on the 'hermeneutic' as well as the causal aspects of psychoanalytic knowledge. Psychoanalysis is only useful to individuals when it relates to their own subjectivity. It holds that understanding should further choice and change for individuals. This is also so at a larger level, for example in Freud's understanding of the benign consequences for society of the lessening of sexual repression.[9] While predictions of a general or 'typical' kind can be made within a psychoanalytic process precise outcomes can never be predicted, in part because the analytic setting is an 'open' and not a 'closed' system.[10] Whereas some psychologies seem to work by standardised diagnoses and treatment objectives, psychoanalysis approaches its patients as, in many respects, unique individuals.

[9] Ernest Gellner's (1995) recognition of the importance of this central insight of Freud gave rise to a much more positive appreciation of his work than was to be found in his earlier *The Psychoanalytic Movement*. (Gellner 1985).

[10] I have argued elsewhere (Rustin 2001) that the consulting room has some of the attributes of the scientific laboratory. Nevertheless there are many 'variables' within this setting that the analyst does not control.

How then should we understand the problems of validating competing psychoanalytic theories? Hinshelwood proposes that a method of testing by falsification could enable fundamental differences between them to be resolved, and thus a unification of theories be achieved. But if Kuhn and Lakatos were right, and the primary containers of psychoanalytic knowledge are holistic research programmes this does not seem to be a plausible prospect. The major traditions within psychoanalysis each constitute in their own way viable 'sub-programmes' of research, generating valid knowledge within their own selected fields of application. Each of them has ways of differentiating between valid and invalid practices. Hinshelwood's own work has shown how the Kleinian sub-field of analysis has added substantially to its scope of knowledge over time, developing more extensive vocabularies of theory and of clinical instances which are available as a 'bank' of conceptual resources for clinicians as they meet the phenomena of their consulting rooms. Some in the natural sciences aspire to the unification of all scientific knowledge usually through the explanatory power of physics[11]. But this seems neither a feasible nor a desirable aspiration for the human sciences, where differences and development are held to be of intrinsic value, and even as among their emancipatory goals. [12]

A Tower of Babel?

A final question to consider is how concerned one should be about the multiplicity of theories there are within psychoanalysis. Is this a weakness which undermines the legitimacy of this field of inquiry, and can the model of theory-testing which Hinshelwood sets out be a means for resolving such differences?

Important as questions of scientific proof are, one should also note that psychoanalysis is not unique among the human sciences in its proliferation of competing perspectives. Sociology, economics, political science and anthropology, not to mention philosophy and history, are all characterised by such differences. The human sciences do not exhibit the same pattern of theoretical convergence as can be seen in the natural

[11] This is a view which the physicist Brian Cox has put forward in his 2013 television series.
[12] Anthony Giddens (1984. P.284) has argued that sociology is characterised by a 'double hermeneutic'. He refers by this to the interactions between the categories used by subjects, and those used by the social scientists who study them. Each influences the other, in a reciprocal process. This description applies to psychoanalysis too, in its relation both to individuals and to society.

sciences, not that it is always a straightforward process in the latter. The main reason for this pattern of differences is the central place of values in the development of the human science disciplines. Different conceptions of human nature and society underlie both the principles of separate 'subjects' (economics and anthropology for example), and conflicting orientations within them. (In sociology, the theoretical differences derived from Marx, Weber and Durkheim have long been significant). Such perspectives remain active over long periods of time because they are resources for conducting arguments about what is desirable and possible in human affairs.[13]

Such differences, shaped by cultural and historical contexts, also explain why there continue to be competing theoretical perspectives in psychoanalysis. It is evident that the framings of, for example, ego psychology and relational psychology in the United States, Lacanian perspectives in France, and object relations approaches in Britain, have been deeply shaped by the dominant social norms and values of their national cultures.

Psychoanalysis is of its nature a highly particular and 'local' activity – it takes place where the patients are, and the mechanisms which bring about convergence in many other scientific fields (such as institutionally funded research institutes and programmes) are weak in psychoanalysis. Within specific psychoanalytic traditions (which are not bounded entirely by national frontiers) procedures for resolving differences of theory and technique often do take an orderly and rational form, through publication and critical professional assessment. It is striking for example how much dialogue and theoretical convergence there has been in Britain between the contemporary Freudian, Independent, and Kleinian Schools. Such dialogue is more difficult to conduct outside of national contexts within which many theoretical presuppositions are shared. It is perhaps among those who take most responsibility for maintaining the international identity of psychoanalysis, in the IPA and EPF for example, that concerns about theoretical disharmony are felt to be most pressing.

[13] Another perspective on the plurality of the sciences arises from Wittgenstein's philosophy. We might suppose that different science are configured as distinct 'language games', linked by 'family resemblances', not by their identity or uniformity. This point was recently made by John Levett in a response to Hinshelwood.

Hinshelwood is among a number of psychoanalysts who have sought to give psychoanalytic thought a greater scientific legitimacy by enhancing its methods of investigation and validation. But the acceptance and standing of psychoanalysis in society may have more to do with dominant social values and cultures, than with questions of scientific method. Psychoanalytic belief in an unconscious mind which is resistant to understanding, and in reflection on states of mind and feeling as a principal resource for human development does not sit easily in every culture.

It is valuable that Bob Hinshelwood has given such careful attention to the issues of scientific method in psychoanalysis, especially at a time when many child psychotherapists in particular are seeking to give a more accountable basis to the development of psychoanalytic knowledge through doctoral and other research programmes.[14] It is particularly valuable in the context of present debates that he has placed his principal emphasis on clinical practice as the primary context of knowledge generation.

[14] The use of 'grounded theory' and similar qualitative research methods is a notable feature of this work, reported in this Journal (Anderson 2006).

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